

problems, will not be discovered at the neonatal examination and are rarely noticed by parents of preschool children. They are some of the most important *raison d'être* of screening programmes. They are rarely diagnosed under 2 years of age,¹⁻³ although the yield between 3 years and school age is high.^{3,4}

If you are a member of a family with an inheritable visual disorder it is likely that you will worry about the visual acuity of your offspring. Furthermore, having one handicap doubles the likelihood of another. This explains the high percentage of children in the study whose initial diagnosis was made by their parents or a paediatrician examining a second major handicap. There is a difference in "ophthalmic health" in metropolitan areas and inner city and rural areas of the world⁵ and thus a need for vigilance even if "families of low socioeconomic state are perfectly capable of making relevant observations about defective vision."

It is wrong to conclude that because most of the children studied were not diagnosed at clinics for developmental screening such clinics make little contribution to the diagnosis of visual problems. After all, maternal suspicion of deafness is an important pointer to early diagnosis,⁶ but no one has suggested that screening for hearing loss should be abandoned.

M POLLAK

London SW16 3EP

- 1 Hatfield EM. In: Barker J, Barmatz H. *Eye function. Pediatric screening tests*. Springfield, Illinois: Charles C Thomas, 1975: 509-10.
- 2 Gardiner PA. The development of vision. In: Pollak, ed. *Studies in developmental paediatrics*. Vol 3. Lancaster: MTP Press, 1982.
- 3 Pollak M. Developmental surveillance. In: Fry J, Gambrill E, Smith R, eds. *Scientific foundations of family medicine*. London: Heinemann, 1978.
- 4 Smith VH. Birmingham orthoptic screening service. In: Smith V, Keen J, eds. *Visual handicap in children*. London: Spastics International, 1979.
- 5 Jones BR. Social responsibilities. *J R Soc Med* 1985;78:358-66.
- 6 Martin JAM, Moore WJ. Childhood deafness in the European community. Brussels: Council of the European Community, 1979. (EUR 6413.)

AUTHORS' REPLY.—The early detection of severe visual defects should be one of the aims of a surveillance programme. In our study most of these were diagnosed by the age of 6 months. We are not, however, making a plea for more accurate screening tests for disabling defects since they are detected by other means.

Suitable tests do exist for screening 3 year old children, but we do not agree that myopia and other refractive errors are "the most important targets." The discovery of a refractive error uncomplicated by amblyopia at the age of 3 years instead of at school entry is of uncertain, but probably marginal, benefit. Detection of amblyopia is more important. We cannot prevent it, so detection and treatment are the best we can offer.

The existence of a suitable test does not guarantee that the screening programme is effective.¹ Screening 3 year olds produces many referrals, but the true yield (the number of new cases requiring treatment) is much smaller. Elementary errors of technique are frequent. For instance, the single letter test is still used, although it has been known for years that it misses or underestimates amblyopia.

The analogy with screening for deafness is unfortunate. The distraction test for deafness at 7-8 months is hardly a success story. Unlike visual screening tests for infants, it is potentially sensitive and specific, but the quality of testing in many places is so low that some authorities are suggesting that it should be discontinued. A badly performed screening test does more harm than good because it

provides false reassurance to worried parents. The traditional skills of taking an accurate history and listening carefully to what parents have to say will contribute more to the early diagnosis of visual defects and other developmental problems than the continued use of unsatisfactory screening tests.

D M B HALL
SUSAN M HALL

Child Development Centre,
St George's Hospital,
London SW17 0QT

- 1 Stewart-Brown SL, Haslum MN, Howlett B. Pre-school vision screening; a service in need of rationalisation. *Arch Dis Child* 1988;63:356-9.

Editorial freedom

SIR,—There are few editors who would quarrel with the cogent points put forward by Dr Stephen Lock (12 March, p 733). From a South African viewpoint there was also a certain wry pleasure in reading that "outsiders who interfere in the internal affairs of others risk rebuke for their presumption and for exacerbating difficulties."

However, his article contains one important error, where he states, "The editor of the *South African Medical Journal* is forbidden to publish letters or articles from members of the National Medical and Dental Association." As the current editor I wish to put the record straight.

The executive committee of the Medical Association of South Africa did pass a resolution advising the editor of the *South African Medical Journal* not to accept any material from the National Medical and Dental Association. The rationale was that as the National Medical and Dental Association appeared to be losing no opportunity to try to discredit the Medical Association of South Africa, both in South Africa and overseas, it made little sense to give free publicity to it in the Medical Association of South Africa's own journal.

This decision, which was originally intended to cover only advertising, was reversed when the threat to editorial independence was perceived. Last year I stated clearly and unequivocally in an editorial¹ that contributions to the *South African Medical Journal* would be judged and accepted at the editor's discretion solely on the basis of their quality and relevance and not on the source from which they emanated. I would like to reassure Dr Lock and the readers of the *BMJ* that that policy has not changed.

N C LEE

South African Medical Journal,
Private Bag X1,
Pinelands 7430,
Cape,
South Africa

- 1 Anonymous. Serving the Truth [Editorial]. *S Afr Med J* 1987;72:371.

**This letter is substantially the same as one published in the *Lancet* of 23 April (p 941). We publish it, with the *Lancet's* permission, to give Dr Lee the chance to reply to a criticism made in the *BMJ*.—ED, *BMJ*.

Risk of upper respiratory tract infection and malaria prophylaxis

SIR,—Drs Pheng Soon Lee and Edwin Y L Lau (26 March, p 893) conclude that differences in the risk of upper respiratory tract infection in the military recruits described was due mainly to the malaria prophylaxis given to one group. This is based on the false presumption that other factors have been compensated for adequately.

Despite the geographical similarity and proximity of the two camps there remains a sufficiently great environmental difference between them for malaria prophylaxis to be required at only one. Surely risks for other infections might also differ.

Other variables which are unaccounted for may be important. Psychological stress is a well recognised cause of immune suppression.¹ Examination students under stress have been shown to have impaired cellular immunity² and reduced production of IgA.³ In the latter study there was a higher incidence of respiratory disease during the period of stress. The impact of personality factors was also assessed. Students exhibiting a "high inhibited need for power" had concentrations of IgA that continued to fall even when the amount of stress decreased. Officer recruits undergoing greater physical and psychological stress than other recruits might be affected similarly.

Greater physical activity has not been excluded as the cause for a higher risk in the officer cadet group. The reasoning that if it were related the increased risk of sprains and of upper respiratory tract infections should be identical is flawed. This is no more logical than saying that jogging should increase the risk of ankle injuries by exactly the same percentage as it increases the risks of being run over by a car. I would be reluctant to draw any conclusion about the relevance of dapsone-pyrimethamine as a cause of immunosuppression on the basis of the authors' findings, and I hope that prescribing practices are not altered.

P J FLEGG

North Manchester General Hospital,
Manchester M8 6RL

- 1 Anonymous. Emotion and immunity [Editorial]. *Lancet* 1985;ii: 133-4.
- 2 Dorian B, Garfinkel P, Brown G, et al. Aberrations in lymphocyte subpopulations and function during psychological stress. *Clin Exp Immunol* 1982;50:132-8.
- 3 Jemmott JB, Borysenko M, Chapman R, et al. Academic stress, power motivation, and decrease in secretion rate of salivary secretory IgA. *Lancet* 1983;ii:1400-2.

Postviral fatigue syndrome

SIR,—Dr A V Mark Hughson (9 April, p 1067) suggests that our arguments have been overtaken by a report which was published after our paper was accepted.¹

In that paper enteroviral infection was found in 17 of the 76 (22%) subjects with postviral fatigue syndrome, and it persisted in only five (7%) at one year. In a second sample of 87 people with postviral fatigue syndrome an enteroviral antigen was detected in 44 (51%), and this remained in 39 (45%) at four months. Despite the detailed virological studies, we think that this investigation exemplifies some of the shortcomings in "the old approach" to this condition. There are scant details on diagnostic criteria and no information on case selection or why there were two samples. Using volunteer control subjects, enlisted by the cases, may have led to biases that could account for the differences. The importance of these results will emerge only with precise information about the population from which cases were drawn and the methods by which they were selected.

One of the main aims of our paper was to encourage discussion of the definition of cases. Dr Hughson points out that we risk underdiagnosing the disorder. This caution is appropriate for research purposes but it does not mean that patients with some features suggesting the syndrome need be denied the best care in clinical settings. This may include prescribing rest for all suspected cases, but such instructions, which have anecdotal support, must be weighed against the potentially severe complications of prolonged inactivity.² The role of controlled exercise in the

treatment of postviral fatigue syndrome is the subject of current investigation.

Drs B D Calder and P J Warnock (9 April, p 1068) seem to disagree that case definition is important, asserting that the search for a diagnostic test is a priority. Clinical scientists need definitions of the disorder which will allow them to focus their gaze more efficiently. We regret misquoting the incidence of postviral fatigue in their study.³ However, their actual figure of 140 cases in a population of 10 000 in six months is even more discrepant when compared with other estimates.⁴ This underlines the urgent need for standardised definitions.

We question the value of distinguishing between "organic" and "non-organic" in the context of this illness. Organic factors (immune state, infection, neuromuscular and neuropsychiatric abnormalities, etc) and non-organic factors (personality, emotional reactions, social and interpersonal problems, etc) interact in subtle and complex ways, and this must be appreciated before rational research can progress.

Research done by sufferers from postviral fatigue syndrome is a delicate issue about which we thought deeply before expressing an opinion. Reviews by Parish⁵ and others are valuable, but clinical research should be carried out by those without the inevitable, albeit unconscious, biases caused by suffering from this condition. Dr Hughson's comparison with Percival Pott is misleading. Pott did not have to consider the complexities of mind-body interaction when contemplating his fractured tibia.

ANTHONY J PELOSI
ANTHONY S DAVID

Institute of Psychiatry,
London SE5 8AF

SIMON WESSELY

National Hospital for Nervous Diseases,
London WC1

- 1 Yousef GE, Bell EJ, Mann GF, *et al*. Chronic enterovirus infection in patients with postviral fatigue syndrome. *Lancet* 1988;i:146-50.
- 2 Edwards RHT. Muscle fatigue and pain. *Acta Med Scand [Suppl]* 1986;711:179-88.
- 3 Calder BD, Warnock PJ, McCartney RA, Bell EJ. Coxsackie B viruses and the postviral syndrome: a prospective study in general practice. *J R Coll Gen Pract* 1987;27:11-4.
- 4 Behan PO, Behan WMH, Bell EJ. The postviral syndrome—an analysis of the findings in 50 cases. *J Infect* 1985;10:211-22.
- 5 Parish JG. Early outbreaks of 'epidemic neuromyasthenia.' *Postgrad Med J* 1978;54:711-7.

Reference values for 75 g oral glucose tolerance test in pregnancy

SIR,—We carried out a similar study in Kuwait to that of Mr M Hatem and colleagues (5 March, p 676) and we came to similar results.¹

In our study 250 unselected pregnant women at 28-32 weeks' gestation had a standard 75 g oral glucose tolerance test. We excluded only those who were known to be diabetic at the time of the test. Venous plasma concentrations were analysed by the glucose oxidase technique as recommended by the World Health Organisation.² The fasting plasma glucose range fell within the World Health Organisation's range for non-pregnant subjects (mean 4.7 mmol/l, mean +2 SD 5.8 mmol/l), while the plasma glucose range two hours after a 75 g glucose load was well above the range proposed (mean 6.7 mmol/l, mean +2.5 SD 9.7 mmol/l). Our values were close to those proposed by the Southampton group.

Furthermore, we divided our population into three groups according to nationality (see table) and found no statistical differences in the reference values between different ethnic groups. In another study we also could not find a statistical difference in random plasma glucose concentrations taken

Mean (SD) glucose values before and after oral glucose tolerance test according to nationality

| Glucose (mmol/l) | Kuwaiti (n=77) | Non-Kuwaiti Arabs (n=125) | Other (n=48) | Total (n=250) |
|------------------|----------------|---------------------------|--------------|---------------|
| Fasting | 4.7 (0.6) | 4.7 (0.6) | 4.7 (0.5) | 4.7 (0.6) |
| After 1 hour | 7.9 (1.9) | 8.1 (1.8) | 8.4 (1.6) | 8.1 (1.8) |
| After 2 hours | 6.5 (1.6) | 6.7 (1.5) | 6.9 (1.5) | 6.7 (1.5) |

within two and more than two hours after a meal between different ethnic groups in our community.³

If other groups studied in different parts of the world yield the same reference values this would support the claim that these are universal values and not ethnically or geographically dependent. It might also suggest that the reference values proposed by the World Health Organisation are rather on the low side for pregnant women.

A A NASRAT

Maternity Hospital,
Safat,
Kuwait

- 1 Nasrat AA, Johnstone FD, Hasan SAM. The value of the traditional diabetic risk factors for performing a glucose tolerance test in pregnancy. *Journal of the Kuwait Medical Association* (in press).
- 2 World Health Organisation Expert Committee on Diabetes Mellitus. *Second report*. Geneva: WHO, 1980. (Technical Report Series No 646.)
- 3 Nasrat AA, Johnstone FD, Hasan SAM. Is random glucose an efficient screening test for abnormal glucose tolerance in pregnancy? *Br J Obstet Gynaecol* (in press).

How informed is signed consent?

SIR,—Dr Niall Warnock (16 April, p 1126) says that "Before any examination, procedure, or surgical operation the doctor requires the informed consent of the patient or he may be guilty of an assault."

While it is true that where no consent at all has been obtained an action would lie in battery, such a charge would be inappropriate where the broad nature and purpose of the operation has been explained. Lord Justice Browne-Wilkinson said in the Sidaway case: "The concept that carrying out an operation constitutes a battery does not accord with common sense," and he pointed out that in the case of *Reibl v Hughes* the Canadian Supreme Court found that even in the absence of informed consent there was no cause of action in trespass to the person. In Britain the proper course of action for a patient who has signed a consent form and subsequently thinks that he has suffered through not being informed adequately is not an action for assault but an action alleging negligence on the grounds of a breach of duty to inform.

I C M PATERSON

Llandaff,
Cardiff CF5 2AB

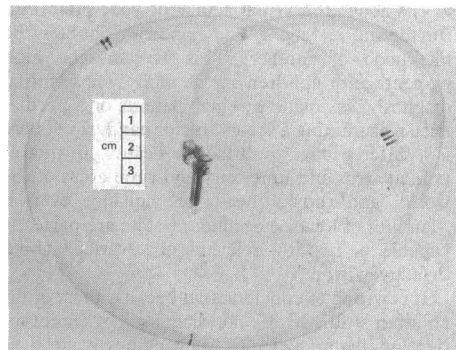
Removal of nasogastric tubes

SIR,—Dr N C Hickey and Miss D C T Watson (9 April, p 1038) were unaware of previous reports of difficulty in removing a nasogastric tube. I know of two reports of knotting of the tubes causing problems.

In one case the tube had been in situ for seven months, and the knot had become rigid because prolonged contact with hydrochloric acid had hydrolysed the ester which is added to the vinyl chloride resin to improve the flexibility of these tubes.^{1,2} The lower end of the tube was stuck in the stomach and was removed at laparotomy. Another

case of knotting caused difficulty during removal in a 5 year old child.³

I have seen two cases of knotted nasogastric tubes.^{4,5} One was removed forcefully in an anaesthetised patient. The second (figure) was dealt with by pushing the tube back down and pulling the knot out of the mouth using sinus forceps.



Nasogastric tube showing knot near tip.

Nurses insert and remove these tubes in the United Kingdom, and only rarely, when an experienced nurse fails to do this, are the medical staff called to assist. It is important that nurses are aware of the potential difficulties with the removal of these tubes.⁵

MEL JONES

Orthopaedic Unit,
Royal Liverpool Children's Hospital,
Alder Hey,
Liverpool L12 2AP

- 1 Hambrick LC. An unusual complication of nasogastric intubation. *Journal of the Medical Association of the State of Alabama* 1983;52:10-12.
- 2 Drenick EJ, Lipset M. Difficulty with removal of plastic nasogastric tube. *JAMA* 1971;218:1573.
- 3 Morris HH. Nasogastric intubation: a potentially knotty problem. *JAMA* 1977;237:1432.
- 4 Jones MW. The knotted nasogastric tube: a simple solution. *Br J Clin Pract* (in press).
- 5 Gaffney L, Jones M. Knotted tubes. *Nursing Times* 1988;84:48.

Medical research

SIR,—Dr Richard Smith's series and leading article (16 April, p 1079) have scarcely touched on the perversion of research achievement into a criterion (in many cases almost the sole criterion) by which applicants are selected for purely clinical posts at senior level.

This obsession has reached epidemic proportions, affecting almost all hospital clinical practice. I am, frankly, sceptical of the accuracy of his story about a surgical registrar being told that he had wasted his time by doing top quality research, but I could give many instances which support the opposite point. A senior medical registrar who has both a PhD and an MD but is now aiming for a clinical career in a district general hospital has been told repeatedly that his curriculum vitae is weak because it contains no recent original publications (within the past five years).

The *BMJ* has emphasised repeatedly the high incidence of statistical weakness in published papers and the large number of "Mickey Mouse" trials published purely to decorate curricula vitae.

It might be argued that doctors benefit by spending time in research, although I am not aware that this has ever been assessed objectively. But it cannot be right to insist that hospital doctors go on and on publishing, irrespective of their long term career aims, at a time when research funding is so seriously deficient. This encourages a criminal waste of resources, which are urgently needed for